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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

THURSDAY, NOVEMBER 5, 1896.

THE PHILOSOPHY OF NATURAL SCIENCE.

Studien zu Methodenlehre und Erkenntnisskritik. Von Friedrich Dreyer. Pp. viii + 223. (Leipzig: Engelmann, 1895.)

Erkenntnistheoretische Grundzüge der Naturwissenschaften und ihre Beziehungen zum Geistesleben der Gegenwart. Allgemein wissenschaftliche Vorträge. Von Dr. P. Volkmann. Pp. xii + 181. (Leipzig: Teubner, 1896.)

IF we were to examine these two works purely from the standpoint of the critical reviewer, we should probably content ourselves by recommending the busy man of science to pass them by. We might, indeed, justify the sternness of our judgment by illustrating the hopelessly involved style of Herr Dreyer—his page-long footnotes on footnotes, his misinterpretations of mathematical and physical theories, and his obvious, but nowhere justified, bias against Darwinism. We might then pass to Dr. Volkmann and show the vagueness of his definitions, the unphilosophical character of his epistemology, and indicate the danger which arises when loose analogies drawn from natural science are applied to other fields of thought. We might not unreasonably conclude with a sigh over the departing glory of German science. We might moan over the death or old age of the giants of a quarter of a century back, and regret that the strong and clear intellects of young Germany seem drawn rather to military and commercial pursuits than to the service of science. That Germany has become the first military, and is rapidly becoming the first commercial nation, are now familiar ideas; but that these victories have been won at the expense of literature and pure science, is an aspect of evolution which other nations are only just beginning to realise, and Germany herself will only realise last of all. It is a subtle qualitative, not a quantitative change which has been going on since 1870 in German science and literature. Few realise it, but it is none the less a reality. It is, perhaps, as well that leadership in all spheres should not fall to one people. From the historical stand-point accordingly, these two books are of interest, for they are very typical of much work which Germany has of recent years put forth. Their authors fully recognise that there are great problems still unsolved in the philosophical basis of the natural sciences, but it cannot be said that they throw any light on the solution of these problems, nay, that they even assist us in their clear enunciation. Herr Dreyer indeed tells us with much truth that:

> "Auf dem Gebiete der Biologie sieht es noch recht wild und windig aus";

but his lengthy defence of "vital force" is hardly calculated to bring more system into biological thought.

Prof. Volkmann is evidently not quite at his ease in his endeavours to define such fundamental concepts as "natural law" and "physical hypothesis." Yet for him natural laws like the law of gravitation lie outside us while the conclusions of mathematics are thought-laws which lie inside us:

> "Diese Naturnothwendigkeiten ausser uns nirgends in Widerspruch treten mit den Denknothwendigkeiten in uns."

This arises apparently from a pre-established harmony the source of which is accounted for in a manner which the writer tells us is the "Kernpunkt meiner erkenntnistheoretischen Studien auf naturwissenschaftlichem Boden." It lies namely in this:

> "dass die Logik in uns ihren Ursprung in dem gesetzmässigen Geschehen der Dinge ausser uns hat, dass die äussere Nothwendigkeit des Naturgeschehens unsere erste und recht eigentliche Lehrmeisterin ist."

We are only given this sentence, without *one* word more description of the process by which such harmony has been established! A poet might have thrown out the idea, but put thus in a scientific treatise it is hardly calculated to help us in clearing up our fundamental notions.

It would, however, be wrong to merely smile over what we feel compelled to term trivialities, or to think that they are solely characteristic of German naturalists. A very superficial study of current English physical and dynamical text-books will suffice to demonstrate how much we ourselves need a thorough *Aufklärung* in our fundamental physical ideas. A mere perusal of current discussions on variation, correlation, panmixia, will avail to show how "right wild and windy" it is in the field of English biology also. This is the second stand-point from which a consideration of the ideas of Dreyer and Volkmann may be of value. Their books may help us to realise the condition of affairs at home, and if we do that we shall hardly find much ground for national self-congratulation.

During the last ten or fifteen years a very great revolution has been more or less silently taking place in the philosophical theory of men of science. The revolution is very far from being complete, for old theories are hard to dissolve when they have crystallised into dogmas. The chaotic definitions of the text-books, the chaotic thinking of too many biological controversialists, are all signs of a transition period, of new ideas struggling with old modes of expression, with an antique terminology suited to a scientific philosophy no longer capable of satisfying modern intellectual needs. But whether we turn to Germany, to France, or to our own country, and study the literature which touches on the *erkenntnis-theoretische* foundations of science, we shall alike be forced to the conclusion that a revolution in scientific thought has been taking place, and that in the minds of the more philosophical men of science, it is already complete. There are many ways of summing up the purport of this revolution. Perhaps the shortest, if not the most expressive, is to say that the old division of science into exact and descriptive sciences is now seen to be illusory. The immense progress made in the first half of this century with theoretical mechanics, the success with which mechanical reasoning was applied in the third quarter of the century to the great physical discoveries of that period, led men of science not unnaturally to postulate mechanism as the basis of all natural processes whatsoever. Particles, molecules, atoms, impacts, vibrations, laws of forces were the forms under which all nature worked, and by which all things were to be *explained*. The dogma that mechanism would explain the universe may have been philosophically absurd, but by concentrating men's thoughts on one method of investigation, it led to a whole round of splendid discoveries. For all but the great leaders of science work under any theory, under any dogma indeed, which has produced and is still capable of producing great results, is far better than the invention of new hypotheses. Fruitful new hypotheses have almost always been the product of master-minds, which have worked out old theories to the point at which they are seen to absolutely contradict phenomena. The 'prentice hand finding some new fact at present unaccounted for by the old-established theory is generally over-hasty with the fabrication of a new hypothesis. The true criterion for the modification or rejection of an established theory which has produced sound scientific results is not its present insufficiency to account for this or that isolated group of phenomena—its insufficiency may arise from the weakness of our analysis, or from want of insight in our application of it—the criterion lies rather in a demonstrable contradiction between the theory and some particular class of phenomena. The existence of such a contradiction can only be satisfactorily proven by a master with the firmest grasp of theory and the fullest appreciation of natural facts. Had the necessity for such a criterion been borne in mind, the field of biology would possibly not now look so "wild and windy." Has there not been a far too ready invention of new hypotheses—on the ground that the theory of natural selection combined with heredity has not hitherto provided a satisfactory account of certain phenomena—while, as a matter of fact, the modification or rejection of such a fruitful theory ought to be solely based on the absolute contradiction of its deductions by our experience of nature?

The whole point is, indeed, well illustrated by Herr Dreyer's onslaught on "die moderne Entwicklungslehre mit ihrem famosen Darwinismus," which "noch so kannibalisch wohl fühlt." It is perfectly easy to show that neither Darwinism[1] nor mechanism in their respective spheres have accounted for anything but a small fringe of organic phenomena. It is quite easy to postulate the possibility of other evolutionary hypotheses and of "vital forces," which in the future may render account of other ranges of phenomena. But take, for example, such a concept as "vital force"—the definition of which is so obscure that it is impossible to assert or deny its existence—and ask what fruitful results it has contributed to biology as compared with what has been achieved by an application of mechanical theory—under which term we should include chemico-physical laws? Herr Dreyer is very stern with Otto Liebmann for ridiculing "vital force"; but when we come to investigate what Herr Dreyer himself understands by "vital force," it appears to be embraced in a *Lebensgesetzlichkeit*, which shall be coordinate with, and not superior to the *physikalische Gesetzlichkeit*. The study of this *Lebensgesetzlichkeit* is to form the science of *Vitalistik*. It would be interesting to know who has been so rash as to deny the existence of yet undiscovered laws of life, which are not identical, but coordinate with already established physical laws. Herr Dreyer's position may be thus summed up: It is not proven that physics can lead us everywhere in the organic field, let us try the fabrication of new hypotheses and build up a new science of vitalistics.

Well, and good! A master-mind may some day propound an hypothesis of value; we should have preferred the statement of single new *Lebensgesetz* to all Herr Dreyer's discussion on *Lebensgesetzlichkeit*. We hold that for the every-day man of science it is better to work a by-no-means exhausted vein of ore, than rush off to the still unworked, but highly-puffed field of vitalistics. Science as well as commerce has its gold reefs—without gold. Nor was the attack of the *so kannibalisch wohl fühlende moderne Entwicklungsmänner* on *Lebenskraft* without its justification. That term embraced an unscientific attempt to slur over ignorance, and encouraged loose thinking by stealing from mechanics in the word "force" some of the clearness and definiteness of mechanical concepts. That no knowledge of *Lebensgesetzlichkeit* came out of its use, can we think be historically proven, nor are we, indeed, prepared to admit that it even acted for a time as a successful bulwark against a materialistic view of life. The materialistic view of life—the theory which would *explain* all organic and inorganic nature by force and matter—has disappeared owing to a wider and more philosophic view of mechanism, and not to the logic of vitalists. It is the physicists, and not the biologists, who have broken down the barrier between the exact and descriptive sciences, and among whom a truer view of mechanism has arisen. The biologists have been only too ready to offer "explanations" of various organic processes by appeal to molecules, centres of force and energies. While they have been attempting a mechanical basis for descriptive science, the physicists have learnt that mechanics, after all, is but a descriptive science itself. "The object of mechanics is to *describe* in the simplest possible fashion the motions which occur in nature." Such was Kirchhoff's definition, and the acceptance of that definition is really the revolution which has been going on in natural science. It is a revolution which cuts at the idea of force as a cause, but sees in it only a measure of change; it is a revolution which thrusts upon us the agnostic position of watching and describing, and which drops *explanation* out of the scientific glossary or defines its old sense entirely away. It is a revolution which again renders for us the motion of a planet every whit as mysterious as the oscillations of protoplasm. We can explain neither, although long study and observation have enabled us to describe one motion in much simpler terms than the other.

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[1] By "Darwinism" may here be broadly understood the theory which supposes evolution to have taken place by natural selection combined with heredity.

Mechanism as the description in the simplest possible terms, not the explanation, of natural motions is a revolutionary definition which at once reduces all physics, chemistry and biology to the same simple footing. In all three sciences it is the sequence of changes in space and time that we endeavour to describe in the simplest language. In doing this, we are inevitably thrown back on geometry and kinematics. The conceptions of these sciences are not identical with real experience; they are based on ideal forms and ideal ratios drawn as limits from our experience of phenomena. But it is in terms of these only that we succeed in describing change, and this geometrical and kinematic description of change, and of repeated sequences of change is what we are to understand by mechanism in its broadest sense. It is this mechanism, which embraces such inventions of the intellect as particles, molecules, atoms and ethers, and describes kinematically, or in terms of the motions it attributes to them, physical phenomena. Organic phenomena may require to be described by other conceptual elements having other modes of motion attributed to them than those which have hitherto been adopted in physical descriptions. But if organic phenomena are to be described scientifically, it must be by a series of symbolic forms moving in time and space. In this sense biology must ultimately become a mechanical science. This does not mean that life can be "explained" by mechanism—on the contrary, mechanism explains nothing, not even physical nature—but that the bulk of natural science is a description of change, of motion in time and space, and that the invention of comprehensive and brief formulæ of motion is the function of mechanics. In this sense it seems impossible to contrast mechanism and "vital force," or to maintain any rigid line of demarcation between the physical and descriptive sciences.

From this standpoint, which we believe to be the firm ground, soon to be left behind by the sea of current *erkenntnistheoretische* controversies, how purely idle does Prof. Volkmann's disquisition on natural laws, rules, hypotheses, and axioms appear! Thus Newton's law of gravitation, the principle of energy, Galilei's law of inertia, he tells us, are *Naturgesetze;* Kepler's laws of planetary motion are merely *Regeln*, and the undulatory theory of light an *Hypothese!* Yet Prof. Volkmann's definitions are worth noticing, because they show us so very clearly the present transitional state of scientific thought.

"Das Naturgesetz bildet den kürzesten und zugleich reichhaltigsten Ausdruck für das, was thatsächlich geschieht und zwar was ausnahmslos geschieht, was geschehen muss" (p. 58).

It is the *sinnliche Wahrnehmung* which changes the *Hypothese* to the *Naturgesetz*. But we must ask, what physicist ever caught a particle, or had a *sinnliche Wahrnehmung* of how two particles, if caught, would attract each other? How is Newton's particle more real than the atom of the chemist? Even Dr. Volkmann admits that the conception of the atom as something *bildlich symbolisch*, is to-day winning ground everywhere. How can we have *sinnliche Wahrnehmung* of the law of inertia? Does it not require the most ideal conceptions of relative motion and of "fixed axes" to at all realise its meaning; and is it then more than a definition of acceleration? We cannot find this law *so einleuchtend* and *so unmittelbar* as Dr. Volkmann believes, nor consider that its essence can be scientifically illustrated by the motion of a locomotive over smooth rails, when the steam is cut off. Indeed our author skates somewhat lightly over the abysmal gulf of the relativity of all motion. He has given up force as a reality, but the influence of relativity on all forms of kinetic energy does not appear to have struck him, and, like many another physicist, he would probably suppose we had some *sinnliche Wahrnehmung* of the absolute in a quantum of energy. The undulatory theory of light was purely hypothesis so long as it was *übersinnliche Anschauung und Vorstellung;* but now that the young German physicist Wiener has photographed light waves, we are told, that it has ceased to be an hypothesis, it has become *eine vollendete Thatsache!*—a law of nature. In not one of such natural laws, however, is there anything of the *muss* of Prof. Volkmann's definition. They are purely *bildlich symbolisch* descriptions of motion, more or less simple, more or less complete and satisfactory.

The reader will notice at once how far many students of science are yet from using the language and appreciating the ideas involved in Kirchhoff's definition of mechanism. Mechanism, whether it be that of the particle, the atom, the ether, or the cell, is a description of motion in the simplest terms the mind can invent, and this description is always in terms of those *bildlich symbolische* elements, which we construct from such ideal sciences as geometry and kinematics. We may attach constants to these elements to be determined by experience, and to be termed charges, masses, &c., and, perhaps, in the distant future, when the science of vitalistics is complete, vital units; but the elements none the less remain *bildlich symbolisch*. They are mental constructs, by which the scientific mind endeavours to describe its past and predict its future experience in the briefest possible terms. It is this creation of *Naturgesetze* by the mind, this invention of brief formulæ, which is at once the glory and limitation of science. The mind does not explain for us what the world of *Dinge an sich*, which we term nature, may be in and for itself; it seeks with all its ingenuity to describe *bildlich symbolisch*, what falls within the limits of its experience. The progress of science lies in the increasing comprehensiveness and brevity of its descriptions.

Prof. Volkmann tells us that:

> "So lange die Naturwissenschaften mit einem inneren Verhältnis zwischen Geist und Natur arbeiteten, war ihr Fortschritt gehemmt" (p. 123).

If this were true, then must natural science and the discovery, or rather *invention* of natural law be for ever retarded, for science must always work at this very relation between mind and nature. It is, however, not the right but the wrong appreciation of the relation of mind to nature which checks scientific progress. The completion of the revolution we have hinted at in this review, so far from being detrimental to natural science, will go a long way towards freeing its workers from the attacks which have been made upon science from more than one quarter, and which have largely arisen from a confusion of the idea of mechanism with some form of materialistic theory. Released from the need of replying to external criticism and attack, science will have the more energy and leisure to progress quietly in its own proper field.

KARL PEARSON.

OUR BOOK SHELF.

Text-Book of Comparative Anatomy. By Dr. Arnold Lang. Translated into English by Henry M. and Matilda Bernard. Part ii. (London: Macmillan and Co., Ltd., 1896.)

THIS volume of Dr. Lang's text-book treats of the Mollusca, Echinodermata, and Enteropneusta. To the first group of animals 283 pages are devoted, and to the latter two 319. The complete and systematic manner in which the structure and relations of the different families and orders described in this work are dealt with, renders each of the three chapters, into which the book is divided, a valuable monograph. Regarding the phylogeny of the Enteropneusta, Dr. Lang states that they "are not closely related to any single large division of the animal kingdom"; his treatment of them in this volume is sufficient evidence that he is not inclined to attach much weight to their supposed affinities with the Chordata. In a short notice it is quite impossible to give any idea of the interesting way in which the book is written. The English translation is all that could be desired; the illustrations are excellent. The arrangement of the subject-matter has been carefully thought out, and reference to any subject is assisted by the use of different kinds of type in the text. A long classified list of the important literature is given at the end of each chapter.

Experience: a Chapter of Prolegomena. By the Rev. Wilfrid Richmond. Pp. iv + 64. (London: Swan Sonnenschein and Co., Ltd., 1896.)

ACCORDING to the author, the initial obstacle to the progress of philosophy is the doctrine that experience cannot give the knowledge of reality—that nothing can be definitely known. This view he demolishes by showing that reality is actually to be found within the field of experience, whence the sensible conclusion is arrived at that "experience is the beginning and end of philosophy."

LETTERS TO THE EDITOR.

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Icelandic Earthquake Recorded at Paris.

LE No. de NATURE du 15 octobre contient une note très intéressante de Dr. J. Stefansson, sur le tremblement de terre survenu en Islande le 26 août dernier.

J'ai l'honneur de vous informer que les courbes relevées, à cette date, au magnétographe de l'Observatoire du Parc Saint-Maur, portent nettement la trace de trois troubles particuliers, paraissant se rattacher à ce phénomène. Le premier s'est produit à 11h. 36m., et les deux autres, qui sont plus accentués, respectivement à 11h. 42m. et 11h. 46m. du soir, temps moyen de Paris.

TH. MOUREAUX.

Observatoire du Parc Saint-Maur, le 29 octobre.

Earth Tremors at Edinburgh between August 25 and September 6.

IN connection with Dr. J. Stefansson's article in NATURE of October 15, on the "Recent Earthquakes in Iceland," it may be of some interest to note that the photo-recording bifilar pendulum, presented to this Observatory by M. d'Abbadie, and placed under my care by Prof. Copeland, exhibits on the dates given by Dr. Stefansson several strongly marked irregularities in the curve, which may possibly have had their origin in the Icelandic disturbances. The following table contains a list of all the pendulum oscillations recorded on the photographs between August 25 and September 6, with the Greenwich Mean Times of their occurrence.

	Earthquakes in Iceland.	Edinburgh pendulum oscillations.		Tilt of pendulum frame.	
		Begin.	End.	″	
Aug. 25		9.25 a.m.	?	0·4	Slight.
,,		4.45 p.m.	5.50 p.m.	0·6	Slight.
26		9.10 a.m.	11.5 a.m.	0·4	Very slight.
,,		0.55 p m.	2.10 p.m.	1·0	Slight, but well-marked.
,,	10.30 p.m.	11.10 p.m.	11.30 p.m.	About 2	Gap.
27		6.35 a.m.	?	1·25	Well-marked.
,,	9 15 a.m.	10.50 a.m.	11.5 a.m.	About 2	Gap.
,,		4.10 p.m.	5.40 p.m.	0·5	Very slight.
29		11.20 a.m.			Several almost imperceptible tremors at irregular intervals.
30			4.30 p.m.		
30		4.30 p.m.			No trace of disturbance, but record not complete.
Sept. 4			4.50 p.m.		
4		4.50 p.m.			Photographic trace a perfectly straight line.
6			0.5 a.m.		
5–6	11.30 p.m.	0.5 a.m.	0.20 a.m.	2 to 3	Gap.
6	2 a.m.	No disturbance here.			
,,		9.5 a.m.	?	1·0	Well-marked oscillation.

The points of special interest in this table are the three violent oscillations which have been designated gaps. These are complete interruptions in the curve, caused apparently by successive waves of sufficient amplitude to produce so rapid an oscillatory motion of the pendulum, that the reflected ray traverses the exposing slit too quickly to leave any photographic trace. The result as seen on the photograph is an abrupt termination to the curve line; then for a period of from five to ten minutes no photographic effect whatever is produced, and for about a similar period, a widened and badly defined trace shows that the ray has oscillated to each side of its normal position, with an amplitude of disturbance at the time when the trace begins to reappear of fully 2″, but which must have been considerably greater at the beginning of the gap, where the record is altogether wanting. After this the mirror comes to rest, and the ordinary trace reappears.

In the case of all the smaller irregularities of the curve, the ray seems to have moved more or less abruptly to a distance from its normal position, after which the mirror gradually settles to rest. Careful measurements give, for the arc through